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(*Edited by D. G. Brinton, M.D., LL. D., D. Sc.*)

THE HIEROGLYPHS OF EASTER ISLAND.

In a previous note (see *Science*, May 8, 1892) I have referred to the curious carved hieroglyphs which the Easter Islanders were accustomed to preserve on batons or narrow tablets. The art is lost, and few of the batons remain, as the present generation burnt up most of them for firewood! A genuine one would now be worth its weight in gold—or, at least, in silver. The last and best work on the translation of the inscriptions has lately been published from the posthumous papers of Bishop Tepano Jaussen, apostolic vicar of Tahiti, who included in his diocese Easter Island also. He secured a few of the tablets, and some intelligent natives read them for him, explaining the meaning of each hieroglyph. These he collated, and they are printed in dictionary form, analyzed as far as possible. They prove to be ideographic in character, and are read *boustrophedon*.

The Bishop took much pains to discover the origin of this writing, sending specimens of it widely over Oceanica for comparison. He finally decided that it was brought "more than a thousand years ago" from the Moluccas and the Spice Islands, almost an identical writing having been discovered on ancient stone monuments in the island of Celebes by Archbishop Claessens, of Batavia.

This excellent study of Jaussen's is a pamphlet of 32 pages, entitled "L'Ile de Paques," and may be had of Ernest Leroux, 28 Rue Bonaparte, Paris. It is indispensable for every student of the subject.

PALÆOLITHIC VERSUS NEOLITHIC.

In the Journal of the Anthropological Institute for February an important article appears from the pen of Prof. Boyd Dawkins, entitled "On the Relation of the Palæolithic to the Neolithic Period." His main point is to prove that nowhere in the Old or New World can we trace the transition in culture between these two periods. Everywhere there seems a gap or hiatus, sharply dividing the two, this break extending also to the fauna of the two epochs.

This opinion was long ago maintained by Mortillet and other eminent archæologists, but has lately been denied by J. Allen Brown and others. Professor Dawkins makes a strong plea for its correctness; but, after all, his argument has the weakness inherent in reasoning *ab ignorantia*. The most he can show is that *not yet* have the steps of the continuity of the periods been demonstrated; while it would surely be difficult for one familiar with the diligent studies of investigators not to be convinced that there is no such sharp line between the two cultures as was once laid down. For instance, all must now concede that palæolithic man made pottery, which was long denied him.

An interesting part of Professor Dawkins's article is that on the so-called palæolithic implements from the Trenton gravels, New Jersey. He has visited that locality himself and collected some of the specimens of which he speaks. His conclusion is, that there is no sufficient evidence for considering any of the Trenton finds as palæolithic; and that the theories which have been built upon them by their finders will have to be discarded. Evidence of another kind than the mere rude form of implements is needed to determine the presence of palæolithic man in America.

THE SO-CALLED "CRIMINAL TYPE."

The all-important question among criminologists is, whether there is a peculiar physical type, which at once marks and condems the habitual criminal. Reference has already been made in these notes to the wide difference of opinion on this subject which obtained at the last International Congress of Criminal Anthropology (see *Science*, Nov. 18, 1892). In a paper read before the Russian Anthropological Society last October, Prof. E. Petri, of St. Petersburg, declared in favor of the reality of the "type," maintaining that it had been denied because of lack of uniformity in modes of measurement, and in the technical nomenclature, as well as from a neglect of proper selection of cases. He argued that a so-called "pure series" of criminal types could be obtained, and would always show clearly defined contrasts to a series of non-criminal individuals.

On the other hand, the legal profession almost unanimously deny the existence of the "type." Take, they say, a dozen criminals as they come into the dock, wash and dress them as neatly, and they will certainly look as well as the dozen men in the jury box impanelled to pronounce upon their misdeeds. To be sure, many criminals are such through want, misery and destitution, and these leave their traces; but as many more have not suffered in this manner; and a large class of crimes demand a well-regulated life for their commission; so the average is maintained. Of course, exception must be made in either case, of mental alienation, idiocy, insanity and the like.

THE PLEIADES IN EARLY ASTRONOMY.

The prominent position which the group of the Pleiades occupies in many early myths and calendars has recently attracted the attention of several writers. Prof. Norman Lockyer, in his "Dawn of Astronomy," shows that the oldest temple on the Acropolis of Athens was oriented to observe the rising of the Pleiades about the year 1530 B.C.; thus connecting the worship of these stars with the primitive religion of the Hellenes.

The subject has been considered at length and with much thoroughness by Dr. Richard Andree, in a late number of Globus (Bd. 64, No. 22). He analyzes the names of the constellation in many languages, and explains its relation in primitive peoples to their calendars and agricultural procedures. He shows that among the most diverse races and in all parts of the globe, these stars have been chosen either to indicate the beginning of years or cycles, or to regulate festivals and recurrent ceremonies.

One who has also given fruitful attention to this question is Mr. R. G. Haliburton, whose results, many of them not yet published, are spoken of by Dr. J. C. Hamilton in the last (fifth) Report of the Canadian Institute of Toronto. He brings together a mass of curious information concerning primitive beliefs about these stars.

The question has special interest in American archæology. At the Anthropological Congress in Chicago last summer, Mrs. Zelia Nuttall read a paper in which she referred to the well-known fact that by these stars the Aztecs regulated their cycle of 52 years. If they had commenced their computation when at that season the Pleiades culminated at midnight, it would be about 4000 years ago,—a deduction which gives rise to interesting speculations.

WHAT IS ARCHÆAN?

BY ALFRED C. LANE, HOUGHTON, L. S., MICHIGAN.

WHEN in the issue of the *Nation* for March 1, 1894, p. 163, I saw my friend Professor Tarr criticised for calling the Huronian Archæan, and saw the reviewer go on to state that the rocks in which magnetic iron ores mostly occur are not undoubted Archæan, I said, "This is *too* much."

The history of the words Azoic and Archæan shows the irony of fate in scientific usage so well as to be worthy the attention of the readers of *Science*.

The term Azoic was originally applied to all the pre-Silurian strata, at first including the intrusives.[1] Later Foster and Whitney excluded them and applied the term to the metamorphic group or formation,[2]—composed of "Gneiss, Mica and Hornblende Slate, Chlorite, Talcose and Argillaceous Slate, and Beds of Quartz and Saccharoidal Marble,"—supposed to be the first detrital rocks, modified by heat. They expressly mention the association of magnetic iron ores with them,[3] and give Logan's division into two groups, which they say they failed to recognize on the south shore of Lake Superior.[4] The Azoic rocks included all rocks below the Potsdam,[5] existing as a geologic system in the Lake Superior region.

Dana objected to the application of the term Azoic,[6] as a misnomer, since there are direct and indirect traces of life in the rocks to which it was applied, and proposed the term Archæan instead. In his use of the term, Archæan is a name applied to one of the four or five primary divisions of geologic time, co-ordinate with the term Palæozoic. In this he has been followed by Geikie and Leconte. From the text-books of these three men, probably ninety-nine per cent of living American geologists have been taught. Moreover, Dana retained the term Azoic,[7] applying it to the earlier part of the Archæan time, and to the latter part applying the term Eozoic in his manual of geology, Archæozoic in a paper published in 1892.[8]

The arrangement in Dana's text-book seemed an admirable one, was widely adopted, and all seemed serene, when trouble arose. The first symptom of it appeared in a circular letter of the Director of the United States Geological Survey, wherein he suggested the division of Geologic time into *ten* periods, to which provisional names were given.[9] The name Archæan was applied to a period below the Cambrian, co-ordinate with it, and separated from it by another period.

No comment nor notice was made on this degradation of the rank of the word, and of course in a provisional scheme it was not necessary. But when in pursuance of this letter the name Archæan was formally applied to a time division earlier than the time of deposition of the clastic rocks older than the Cambrian, and co-ordinate with Cambrian,[10] the mischief was done.

The term Archæan, introduced to replace Azoic as a misnomer, has been so changed in application by the United States Geological Survey as to include only rocks which cannot but be Azoic.

There certainly could have been no life before the beginning of sedimentation.

The only reason for this change, that I know of, is given by Van Hise in the following words:[11]

"As here used the term Archæan is restricted to this fundamental complex. It is no longer possible to regard as a unit or treat together all pre-Cambrian rocks. The rocks included in the fundamental complex are everywhere called Azoic or Archæan. The crystallines and semi-crystallines above this complex, often called Archæan, must be distributed from the Devonian or later to the pre-Cambrian. It is clear that if Archæan is to remain a serviceable term it must be restricted to some unit. Such a unit is the fundamental complex, and to it this term is most appropriate."

Comment is hardly needful, in view of the fact that for some four hundred and seventy eight pages Professor Van Hise has been treating all the pre-Cambrian rocks together, and that, as he avers, "it is impossible to make a wholly satisfactory theoretical definition of the Archæan" (as he uses it).

I should perhaps add that while I still think that the Archæan will "remain a serviceable term," if retained in the sense in which it was proposed, and is used by all the leading text-books, not only in America, but also in England (Geikie), and Germany (Credner and Neumayr) and is employed by the Neues Jahrbuch für Mineralogie, Geologie, etc. (as one of four or five divisions of geologic time co-ordinate with Palæozoic), I do not here question the appropriateness of its division, nor attempt to combat the arguments so ably urged by Van Hise[12] for the crustal or sub-crustal character of the fundamental complex. I merely cannot see why the time-division given by Dana is not satisfactory, and why the time previous to the formation of clastics should not be called Azoic. Then for a parallel formation term, according to the principles of that dual nomenclature, rightly proposed by H. S. Williams, Van Hise's term Basement Complex seems to me very appropriate. Possibly Basal, as somewhat shorter, and in adjectival form more correspondent with other terms, might be better. The age of the Basal, so far as formed by subcrustal consolidation, might not be altogether Azoic, but it would all belong to one formation.

[1] Van Hise, Archæan and Algonkian, Bulletin No. 86, United States Geological Survey, 1892, p. 470; but compare Foster and Whitney, p. 3.
[2] Foster and Whitney, Lake Superior, part ii., 1851, p. 2.
[3] *Ibid.*, p. 8.
[4] *Ibid.*, p. 11; compare Van Hise, loc. cit., p. 470.
[5] *Ibid.*, p. 2.
[6] Van Hise, loc. cit., pp. 394, 473.
[7] Manual of Geology, 1880, p. 140.
[8] Summarized in Van Hise, loc. cit., p. 469.
[9] Tenth Annual Report of the Director, United States Geological Survey, 1890, p. 59.
[10] *Ibid.*, p. 66.
[11] Van Hise, loc. cit., p. 478.
[12] Van Hise, *American Journal of Geology*, vol. i., p. 113.

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THE PROGRESS OF ASTRONOMY IN 1893.

BY GEORGE A. HILL, WASHINGTON, D. C.

THE year 1893 beheld a most rapid progress in all branches of astronomy. It will be our purpose to call attention to the more important advancements that have been made, and also to items of especial interest, as a complete resumé of the subject would reach far beyond the limits of this article.

In instrumental equipment we have passed beyond our present apparatus, and we are launching out upon a most wonderful improvement in instruments and their accessories. The mounting for the great telescope for the Yerkes Observatory has been completed, and was on exhibition at the World's Fair. The tube will be at least sixty-four feet long. The object glass of forty inches clear aperature is a reality, and Alvan Clark, its makers has said that it will be finished the coming summer.

Brashear has completed the star spectroscope, and is about to commence the solar spectroscope and the spectroheliograph.

Professor Pickering has received from the maker, Clark, a twenty-four inch photographic telescope, the gift of Miss Bruce, of New York. The telescope will be mounted on a high mountain of the Andes, in Peru, and from that elevation its whole energy will be concentrated upon a photographic map of the heavens.

Father Algúe has just departed for Manila, in the Phillippine Islands, at which point he will install an astronomical observatory. The most important instrument that accompanies Father Algúe is an equatorial, with an object glass of nearly twenty inches clear aperature. The mounting is by Saegmuller, of Washington, D. C., the object glass by Mertz, of Munich, and is a companion to that at Strassburg, and the one at Milan, which has been productive of so many interesting observations in the hands of Schiaparelli in his measurement of double stars, and the markings upon the planets, especially Mars. It was the writer's pleasure to recently wish Father Algúe God-speed in his long journey and a rich measure of success from the results of his observations to be made in that far-off land.

The Greenwich Observatory has accepted from the maker, Sir Howard Grubb, an equatorial with an object glass of twenty-eight inches aperature, and the Astronomer Royal reports that it is satisfactory.

The United States Naval Observatory, one of the most magnificent and complete astronomical edifices in the world, both in instrumental equipment and offices for the astronomers, was during the year formally opened.

The Dudley Observatory at Albany, N. Y., has, under the wise administration of its director, Professor Lewis Boss, been moved to a more desirable site, a new twelve-inch object glass by Brashear has been supplied, the other instruments repaired and refitted with all modern conveniences, and a substantial residence built for the director in proximity to the instruments.

Dr. S. C. Chandler, of Cambridge, Mass., has presented to the astronomical world a remarkable series of papers, commenced in 1891, and continued through the past year. The exhaustive investigation undertaken by that gentleman has lead to the proof that the earth's axis of rotation is not invariable, but that a variation of latitude does exist. Dr. Chandler has made a thorough discussion of all astronomical observations, which bear upon the determination of latitude from the time of Bradley down to the present date, and any one who has read his papers must be convinced that he has almost in sight the law that controls the variation in latitude. He has made a thorough discussion of the work of Struve, Peters, Gyldén and Nyrén at Pulkova, with both the prime vertical and vertical circle instruments; observations made at Washington with the prime vertical in 1862–66; Küstner's zenith telescope work; observations made at Cambridge, Leyden, Melbourne and Greenwich; Doolittle's zenith telescope work, and Comstock's and Brown's meridian circle observations at Madison. All these have been discussed in a masterly manner, and from them has been established the fact that the revolution of the earth's pole occupies a period of about 427 days, moving from west to east, the amplitude being a variable and probably entangled with a yearly period.

In connection with what has been said, the reader will find in *Nature*, vol. XLVIII., page 451, a very interesting paper by Professor C. L. Doolittle, given as his address as Vice-President of Section A (Astronomy) at the last meeting of the American Association for the Advancement of Science, held last summer at Madison. Professor Doolittle's paper covers every historical fact connected with the subject of the variation of latitude, besides giving valuable information from the results of his zenith telescope observations carried on so many years at Bethlehem, Pa.

Mr. S. Kostinsky, of the Pulkova Observatory, presented last February to the Imperial Academy of Sciences, St. Petersbourg, a paper containing observations made of close zenith stars with the prime vertical transit instrument at that observatory, for the express purpose of determining the amplitude and period of the variation in latitude, recently dug up by Chandler. From that series an aplitude of $0.60''$, with a period of 412 days, is demonstrated. Mr. Kostinsky's paper will be found in Bulletine de l'Académie Impériale des Sciences de St. Petersbourg, tome VII., page 367.

Upon exhibition at the World's Fair was a new form of pendulum devised by Sigmund Riefler, of Munich. Mr. Leman read a very interesting paper before the Congress of Astronomy held in Chicago, which was a description of the pendulum. His paper appeared in the December number of *Astronomy and Astro-Physics*. Accompanying the article was a table extracted from the records of the Royal Observatory at Munich, giving the daily rate of the clock controlled by one of Mr. Riefler's pendulums. The period covered by the table is from September 1, 1891, to September 2, 1892, or one year. Dr. Seeliger, the Director of the Observatory, states, in forwarding the table, that with a variation of temperature up to 30° centigrade no influence worth mentioning on the rate of the clock could be perceived. The mean daily

observed rate was between +0.01 and +0.03 of a second; when reduced to a constant barometer reading it varied between −0.002 to +0.007 of a second. If all of Mr. Riefler's clocks will keep up such a record as that quoted, then it is to be hoped that a number of them will remain in this country. I have advisedly omitted to describe the mechanical ideas contained, as it would be totally unsatisfactory unless accompanied by a drawing of the escapement, which is not at my command.

Dr. Vogel, of Potsdam, has a highly interesting paper in the *Abhandlungen der Konigl. Preuss*, Akademie der Wessenchaften, Berlin, containing the results of his spectroscopic studies upon the new star in Aurigae, discovered by Anderson in 1892. Dr. Vogel's paper is too extensive to more than mention the most important facts. The spectrum of the new star extended far into the violet and showed many broad and bright bands. It was evident to him that the spectrum was not that of a single body, but was made of the superposed and relatively placed spectra of two bodies, which were moving with great velocity. Dr. Vogel then gives a carefully prepared description of the spectrum as it appeared during each night's observation. Professor Barnard, observing the star with the thirty-six inch at the Lick Observatory, found that it appeared nebulous.

A very interesting series of papers have been printed in the *Astronomical Journal*, containing a discussion by Mr. C. L. Poor, of the orbit of comet 1889, V. A rather faint telescopic comet was discovered by Mr. W. R. Brooks on July 6, 1889. The object would not have attracted much attention from astronomical students were it not for the fact that a short series of observations indicated that another periodic comet had been added to the list. That indefatigable worker, Dr. S. C. Chandler, found that the comet had been greatly perturbed by Jupiter in 1886, so much so as to change the orbit from one of twenty-six years to that of between six and seven. Mr. Poor, however, from a much longer series of observations found that previous to 1886 the comet had a period of over forty years. Recently Dr. Julius Bauschinger in a work entitled "Untersuchungen über deu periodischen kometen, 1889 V (Brooks)" I Theil., has from all the published observations, covering a period of over three quarters of a year, obtained a definitive set of elements, and upon those data Mr. Poor has given the orbit of the comet a most exhaustive investigation. Dr. Chandler had attemped to prove the identity of comet Brooks with that of Lexell's comet, 1770, but Mr. Poor seems to be of the opinion, after weighing carefully all the facts, that comet Brooks is not the same as comet Lexell. Dr. Wilson in *Astronomy and Astro-Physics* for November gave a complete resumé of the subject, accompanied with drawings showing the orbit of the comet before and after its approach within the action of Jupiter.

A new catalogue of colored stars has appeared, prepared by Herr Fredrich Krüger, of Kiel. It contains all the stars north of 23°, south declination, that are of a yellow or reddish color, or those that are remarkable through the existence of absorption spectra. The introduction contains a reference to previous catalogues, the work of different observers, their instruments and methods, etc. The catalogue is indeed a valuable one. In No. 300 of the *Astronomical Journal* will be found a second catalogue of variable stars, prepared by Dr. Chandler. He has used the latest observations and has computed the elements for each.

Judging from the earnestness displayed by Charlois and Wolf, the number of asteroids that will be discovered in the future will only be limited by the amount of patience displayed by those observers in exposing their photographic plates to sky. Up to August 19, the orbits of 372 of those strange little bodies scattered between Mars and Jupiter have been computed. In the present year an even forty have been added to our store-house of knowledge, although some of them may be identified as old ones.

During the year three comets were discovered. The first one was the first return of comet of Finlay. In 1886 Mr. Finlay, an astronomer at the Cape of Good Hope, discovered a small comet, which soon proved to be a short period one, and its return was predicted to occur in the spring of the past year. M. Schulof, of the Bureau des Longitudes, Paris, took the orbit of the comet in hand, and the result of his work will be found in the current numbers of the *Bulletin Astronomique*. Mr. Finlay again found the comet, on May 17, very near its predicted place.

In the first week in last July a bright comet appeared on the eastern side of the sun. Its discovery has been claimed by many. It was seen simultaneously at a number of places in Spain, France, and the United States. It shone as a hazy star of the third magnitude, and as its motion was very rapid, upon coming out from the twilight, a tail of ten or twelve degrees in length was seen. Its orbit does not differ from a parabola.

The last comet of the year was found by Brooks, on October 17. It never became very bright, but in December it was well placed for observing, as it was then subpolar for stations in central northern latitudes.

On April 19 a total eclipse of the sun took place, the line of totality first touching the western shore of South America at Saco, then passing northeastward across the continent, cutting Africa to the south of Cape de Verde Islands. It was observed by two British parties, one at Fundium, west Africa, the other at Para Curu, Brazil. A French expedition was also located at Fundium, another at Joal. Two American parties, one the representative of Harvard College, the other of the Lick Observatory, were stationed in Chili. All observers were fortunate in having good weather. All parties gave their attention to photographing the corona, and many of the negatives show a great deal of detail. The corona as seen in April, 1893, was very different from that seen on Jan. 1 or Dec. 22, 1889. It more nearly resembled that of 1871 in the distribution of the coronal streamers.

During the year appeared a volume which is a monument to the work of any astronomer. It is the Cordoba Durchmusterung, by Dr. John M. Thome, the director of the Cordoba Observatory, in the Argentine Republic. The volume contains the brightness and position of every fixed star down to the tenth magnitude between 22° and 42°, south declination. This valuable work contains the first part of the great undertaking by Dr. Thome, namely, the continuation of Schönfeld's Durchmusterung southward. This has been accomplished for the region between 22°, Schönfeld's lower limit, and that of 42°. These are contained in Vol. XVI. of the Cordoba observations, and give the position of 179,800 stars; the second instalment, forming Vol. XVII., will contain 160,580 stars. The latter volume will appear during the year. The great work carried on by Dr. Thome is truly heroic, and his labors have been since 1885 devoted distinctly to the completion of one grand idea, a durchmusterung from pole to pole.

During the year Mr. Edwin F. Sawyer published a catalogue of the magnitudes of southern stars from 0° to 30°, south declination. The catalogue contains the magnitudes of 3415 stars; the average number of observations for each star was four. The system of magnitudes adopted was that of the Uranometria Argentina. It appears to be the most important and accurate photometrical work which has appeared in a decade.

What has gone before has been the progress of astronomy in the year just past.

In closing we cannot omit to offer our tribute to those that have past from the field, and to mention the honors that have been received by the living.

The death last May of the Rev. Charles Pritchard, D.D., F.R.S., late Savilian Professor of Astronomy and Director of the Observatory at Oxford, removed one who left a deep impression, if not in astronomical investigations, in his students that are taking up the work as he had taught them. Dr. Pritchard was a broad-minded man, honest in his purpose, upright in his character, sensitive to the feelings of his students and active in his investigations. Mr. W. E. Plummer has given in *Observatory* for July, 1893, a sketch of Dr. Pritchard's life, a pure, noble one.

In the death of Dr. Adolp Steinheil the astronomical world has lost one who was an authority upon the construction and grinding of object glasses. His death took place at Munich on Nov. 4 last.

On Dec. 6, death once more entered the flock and took from us that eminent astronomer, mathematician and historian of science, Dr. Rudolf Wolf, of the Zurich Observatory. Since 1855 he had been the Director of that observatory, as well as Professor in the Polytechnicum. Dr. Wolf's pen was well handled, as may be seen in his writings, embracing as they do researches in pure mathematics, astronomy, physics and the history of science. In 1852 appeared the first edition of "Taschenbuch of Mathematics, Physics, Geodesy and Astronomy"; in 1858–61 the four volumes of biographies of Swiss men of science; in 1869–72 his "Handbuch der Mathematik, etc.," in two volumes. Probably his most extensive work, the "Handbuch der Astronomie, ihrer Geschichte und Litteratur," has just been completed. All astronomers have at times turned with interest to his "Astronomische Mittheilaugen," commenced in 1856 and continued to the present time. In 1877 he published his "History of Recent Astronomy, Especially in Germany." Dr. Wolf won his reputation in his important discoveries and researches relative to the solar spots. In *Astronomical Journal* No. 309 will be found a very interesting sketch of his life, and from which we have drawn many of our statements as to his work in astronomy.

The December number of *L'Astronomie* conveys the information that the Academy of Sciences of France had decided to confer the Arogo gold medal upon Professor Asaph Hall for his discovery of the satellites of Mars, and upon Professor E. E. Barnard for his discovery of the fifth satellite of Jupiter. The Arogo medal has been conferred only once before, to the illustrious Leverrier, in recognition of his discussion that lead to the discovery of Neptune.

SUNSPOTS AND METEOROLOGY.

BY H. A. HAZEN, WASHINGTON, D. C.

THE spots on the sun are disturbances or storms largely of electric origin. They have a drift or motion along the surface at the equator of 867′ per day, and at lat. 14°,851′. It is known that these spots have a definite period of about eleven years. They have an intimate relation to the fluctuations of magnetic declination, and to the appearance of auroras. An interesting discussion of the question as to the immediate transmission to the earth of an influence from a solar outburst will be found in recent numbers of *Nature*. From time to time serious efforts have been made to show that there is a close connection between sunspots and terrestrial disturbances. It should be noted that mere coincidences between phenomena, no matter how often they are observed, can have little weight in establishing a relationship, unless it can be shown, *a priori*, how or why there is a connection. On the strength of mere coincidences, the ancients were justified in holding the view that the earth was stationary, and all the universe rolled about it, producing the "music of the spheres."

In studies of coincidences, it is very easy to ignore the cases which disagree with a preconceived relation, and to magnify those that do agree. For example: it is believed by some that sunspots have an influence at the earth only as they first appear by rotation on the eastern limb of the sun, and this, too, in face of the fact that some of our most brilliant auroras have appeared with large spots near the centre of the sun's disc. In Nov., 1882, a spot at the centre of the sun could be seen with the unaided eye, and yet it was assumed that the aurora then seen, one of the finest, was due to a few small faculæ just appearing on the sun. No theory of this kind will hold unless it can be shown why the spot loses its influence after it is a few days old. The present writer is well aware that those who adopt this view, that the sunspot has influence only at the time of its first appearance by solar rotation, will not submit to the following crucial test, which is equally applicable to all discussions of this kind: Decide upon the three days in each rotation when the influence should have been the greatest, and also the three when it was weakest, then determine independently the actual meteorologic conditions over any definite portion of the earth, say, for the lake region, or the Ohio valley, or even the whole United States, as regards storms, cold waves, or any other definite phenomenon. A comparison of the data would at once show the value of this supposed influence and a relationship, if there is one.

Attempt has been made also to determine a periodic effect from these spots, or from the source of energy behind them, at specific solar meridians. At first sight, since, as we have already seen, the spots have a different rotation period at different solar latitudes, it would seem as though such a period could not possibly be established. If, however, there are certain influences at specific solar meridians tending to increase spots independently of their drift, such influences may be felt at the earth and would also show themselves by an increase or diminution in the spots whenever that meridian of the sun was at the centre, though it should be noted that this influence can hardly concentrate itself at any one meridian, but must be felt over many days, unless we consider that the sun's influence has tides or blazes up at certain definite times in each rotation. Spots might drift at any velocity, but when they approached such a meridian they would show it in their appearance all the way from the sun's equator to their limits on either side. Various periods have been set for these meridians of maximum influence; some of these are as follows: 25, 25.5, 26, 27, 27 1/4 days, etc. In order to test this question, it has been customary to arrange spot areas according to the selected or determined period for 100 rotations or so, and then to average each day by itself. There is nothing so easy as to arrange figures in this way, but the procedure is fraught with grave danger, as we are liable to cover up just the most important fluctuations by an indiscriminate mean, and our final result shows such microscopic fluctuations that we are tempted to magnify them by an easy multiplication.

A common argument among advocates of such methods, to account for failures in obtaining a period in the past, is that the exact period has not been discovered. One investigator has developed a period of 27.27778 or 27 d. 6 h. 40 m.; another, in studying shorter periods, has made two, one of 7.26917 days (7d. 6.46 h.), and another of 6.16417 days (6 d. 3.94 h.) and so on. This point is not well taken, however, for a little experience with

figures in a known period, by combining them according to a slightly different law, will show at once, not only that we have not the right period, but what that exact period is. For example, take 27 figures in a definite progression and arrange them according to a period of 27 1/3 days, average each 6 periods in succession by themselves and we shall find that each group has its maximum point retarded by two days from the preceding. In meteorologic work it is better to average in groups of 6 and 7 periods in each year, as this eliminates the seasonal effect.

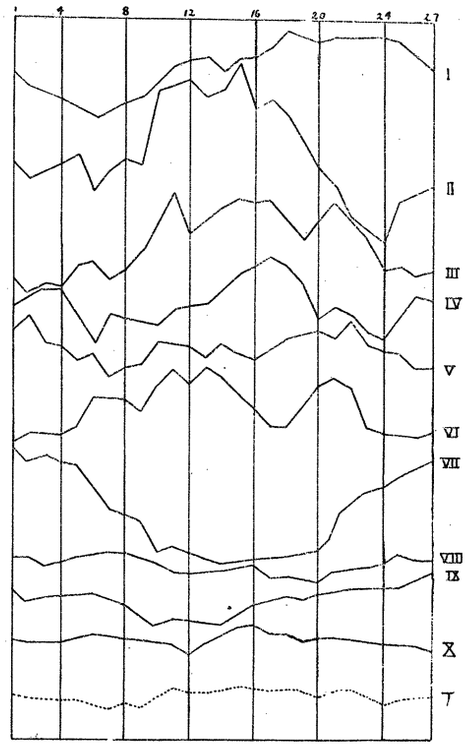
The best data to study the sunspot period are contained in photographs of the sun taken at Greenwich and in India, which together give a superb record for nearly every day of the year. We shall avoid difficulties if we average these figures in groups of ten rotations each, and we would naturally expect to find the influence in each group. If we do not find a thread running through all these groups, it would be pretty good evidence that we cannot get at the influence, even if there is any, in this way, but that it was either entirely masked by other forces, or else was too insignificant to show itself.

I have arranged the sunspot areas from April, 1881, to 1889 in different periods of 25, 25.5, 27 and so on days and in groups of ten rotations each, and have projected curves from these. The curves for the period of 27 days are given here. These curves present the actual data without smoothing or rounding, as this best shows the precise fluctuations. The light vertical lines show the first, fourth, eighth, etc., day of the rotation. Each group of ten rotations has a Roman numeral at the end, and the last (T) is the mean of the 10 groups, or of 100 rotations. We see at a glance that these groups have no common thread at any specific day of the period running through all, and this shows that nothing can be hoped for in this study unless we can first eliminate some of the influences which mask the real effect at the solar meridian. The more reasonable hypothesis is that there is no definite period in this influence.

But this is not all. The last curve (T) shows how exceedingly slight the effect must be, and a few more groups would have practically removed even these slight fluctuations. Again, we see that the maximum and minimum points in (T) are often due to a marked fluctuation in one, or at most in two, of the individual groups, for example: the maximum point in (T) near the 16th day is due entirely to the marked fluctuation in II. at the same day, and, if that group, or perhaps two rotations of the ten in the group, were omitted, this fluctuation would practically disappear. It would seem as though the evidence was conclusive that these crests and hollows in (T) are entirely fortuitous and do not show any influence recurring from rotation to rotation or any physical connection in the fluctuations of the different groups.

Precisely the same reasoning will apply with still greater force to similar studies of meteorologic elements. Here we have a great many local causes modifying the original cosmical influence, and these must be eliminated first. The temperature of the air, for example, is affected by the onward progress of hot and cold waves, by clouds, which serve to diminish the sun's heat by day and terrestial radiation by night, by moisture, by rainfall, by wind, etc. Before we could attempt a study for periods in this element, it would be necessary to take out these effects or to show that they are overborne by the greater cosmic influence.

I do not think that this negative result should lead us to conclude that extra terrestrial forces, aside from heat, have no marked effect upon our weather, but simply that this is not the proper method of getting at these influences. The evidence of such effects is overwhelming, and we may hope for the greatest advances in meteorology in the near future along these lines of research, that is, in developing extra terrestrial influences. Whence comes the enormous amount of electricity stored in the atmosphere at heights of 5,000 to 10,000 ft.? An answer to this single question would be of inestimable value to meteorology. It would seem to be a direct effect from the sun. The present lively discussion and most careful studies upon the influence of electricity in vapor condensation are gratifying and give an earnest of what may still be hoped for. The recent pilot balloon ascensions in Paris up to nearly 10 miles open up a new field for the meteorology of the future. These conditions at 10 and 15 miles must be due to extra terrestrial forces. It is claimed by many that the heat of the sun has no effect upon the air at these enormous heights, and yet, there are indications of most marked fluctuations of temperature on the passage of high and low areas. It has been well established that on the



approach and passage of a low area over Pike's Peak, Sonnblick and other high mountains there is a marked rise in temperature, and the fluctuation is generally greater above than below.

On Mar. 21, 1893, in Paris, a balloon reaching nearly ten miles in the centre of a high showed a diminution of 1° F. in 310 ft. Sep. 17, the same balloon near a low showed a diminution of 1° F. in about 380 ft., or pecisely the same law as on mountains.

Meteorology has been woefully hampered in the past by the theory that all weather changes are due to the heating or cooling, and flowing, or drifting of masses of air comparatively near the earth. Let us look away from these minor conditions and get an insight into the real causes of these conditions, or what is back of them. Let us broaden our view and see how extra terrestrial forces are continually acting upon the upper limits of our atmosphere as well as upon the lower layers and are feeding or generating our storms, cold waves, etc. Let us cut loose from the idea that all our weather conditions are borne along by air currents, for it is plain that this is impossible, since

the velocity of these currents at 5,000 ft. is double that at the earth, and any storm or high area would be very quickly disintegrated by such action.

AUTUMN COLORING OF LEAVES.

BY G. W. M'CLUER, CHAMPAIGN, ILL.

THE subject of this paper was suggested to me more than a year ago by some highly colored leaves of the common red currant that I noticed while rambling in the woods near Urbana.

I shall not take up my notes in the order in which they were made, or of their importance, but shall begin with the smallest.

In the leaves of several species of trees where a principal vein has been broken, the part toward the tip of the leaf has colored earlier and more highly than the rest. Not unfrequently, in lobed leaves, the lobe with the broken rib has been the only part of the leaf that showed any coloring. Leaves torn from the margin to the midrib were not changed as a result of the injury.

The species in which parts of leaves have been seen to color as a result of this injury are soft and hard maple, tulip, white, red and scarlet oak and cottonwood, and blackberry. Probably all plants that color at all would show the same thing.

Carrying the observation a little further, branches of many kinds of trees and shrubs that have been girdled or otherwise injured, have been found to color earlier and usually, though not always, much more highly above the injured or girdled part than below, or than other branches on the same tree that were not injured. As noticed before, the thing that first called my attention to this subject was finding a clump of the common red currant with the leaves on the tips of several twigs colored dark crimson. An examination showed that all the twigs with colored leaves had been girdled by some insect while none of those still green were. Girdled grape branches frequently have very highly colored leaves, but I have never seen them colored on branches that were not injured. Girdled or injured branches with highly colored leaves have been noticed on all our native maples, on cottonwood, elm, wild black cherry, oak, pear and plum.

A large branch on a hard maple on one of our streets used to attract my attention, and I sometimes wondered if its habit of early coloring could not be perpetuated by budding from it. It was finally broken off and was found to be eaten by borers and partly decayed. Branches on some of the hard maples round which wires have been tightly drawn have colored earlier than the rest of the trees. In a few cases where forked branches have split, the one nearest off has been found to be colored more than the other.

The next step would be to find the whole tree more highly colored than those around it on account of some injury to the trunk. Believing that a variety of trees might be produced and propagated that would color regularly every fall, I have watched for such as might offer a good chance to try the experiment. In the artificial forest belonging to the University is a plat of hard maple that has attracted my attention several times, but in looking at trees that were especially brilliant, it was found that all of them had some noticeable defect in the trunk; usually a place where the bark had been destroyed on one side of the tree and the wood left exposed so that it died. In a group of hard maples like these there will frequently be found in the fall all gradations of color from plain green to bright yellow or red, at the same time. There is an individuality in time, depth, and shade of color that is entirely independent of the condition of the tree itself.

Of four young soft maple trees near my house that were planted at about the same time, one has been allowed to get full of borers, and the trunk is so badly eaten that the tree will probably die before a great while, but last fall it was more highly colored than I had ever seen a soft maple before, and the leaves were all off of it before the others had fairly begun to turn. The brightest colored red oak yet seen had a barbed wire drawn tightly around it at about three feet from the ground, and the brightest Virginia creeper had had the bark knocked off in some way.

It is now only one more step to the roots of the trees. We have on the University farm a plat of two year old white oaks that have been grown for planting in the artificial forest. As a preparation for their removal, the top roots were cut last fall. The work was begun while the leaves were still green; when it was about half done, it was stopped and not completed until two weeks later. Within a week after the roots of the first had been cut, there was a very marked change in the foliage. The root pruned trees were all highly colored, while of those not pruned there was only occasionally one that had begun to color.

It is a matter of common observation that trees on hilly land color more deeply than those on level land, but it is not so frequently reported that they also color earlier, though the latter is just as much a fact as the former. Trees on thin land color earlier than the same species on rich land.

It is a common practice among nursery men to stop cultivation before the end of summer and allow the weeds and grass to grow, so that the trees will ripen up their wood earlier and more perfectly. Cultivated land holds moisture longer than uncultivated, and when the uncultivated land has a coat of weeds and grass, the loss of moisture is still more rapid.

It is a very common thing to see a clump of wheat or oats standing out at the edge of a field where it has things more to itself, that is much greener than the general field; or to see thick and thin spots in a field, and the thick always ripe earlier than the thin. Corn grown very thick ripens earlier than corn grown thin. A plat of corn on the Experiment Station grounds, that had no cultivation this past year, the weeds being allowed to grow at will, ripened earlier than cultivated plats of the same variety on each side of it. This shows that the time of ripening depends on other circumstances than the individual character of the plant.

It is not uncommon for our apple trees to shed many of their leaves during a season of severe drouth. If this is followed by heavy rains and warm weather, blossoms and new leaves are very apt to make their appearance. The age of a tree makes a difference in the length of time it uses to complete its season's growth. Young trees, under like conditions, grow longer than older ones, and this is not a matter of shade either, for it is seen more decidedly in nursery stock, as compared with older orchard trees, than in trees in the forest. Sprouts from the stump of a tree that has been cut down grow later than trees of the same species near then. Water-sprouts in apple and other trees grow later than the normal branches; in fact there is but little difference in this respect between the water-sprout and the nursery grown apple tree. Both may be seen until the early winter with tufts of unripened leaves still adhering to their tips.

This thought leads naturally to the subject of determinate and indeterminate growth of plants.

Some species, as the oaks and ashes, will start out a shoot in the spring that reaches its full length in a short time, and the leaves expand and the wood hardens after-

ward. The whole plant is then apparently at a standstill for a time, but when it has matured its buds it starts out again and makes another growth just like the first, though not usually so long. In this way the tree may make two, three, or more distinct growths the same year, depending on the age of the tree, and other conditions. Trees that make their growth continuously stretch out most rapidly at first, becoming slower and slower until growth ceases. The number of buds, leaves or joints which such a branch will form depends on circumstances; its position on the tree, the character of the season, etc. Terminal branches not only grow longer, in both senses, than lateral ones, but produce more buds. Water-sprouts grow longer and produce more buds than either. If a branch is cut off so that all the growth is forced into what would have made a short lateral, it then takes the nature of a terminal branch, and what would otherwise have made a bud for next year's growth, is developed right along into this year's branch, which grows more rapidly, makes more leaves in the same length of time, and also ripens later in the season.

While no notes have been kept, it has seemed to me that our trees growing in a deep rich soil have colored more after a dry season than after a wet one. There can be no question that there is a great difference in the intensity of the color which the same tree will take on in different seasons.

We have in the experimental orchard two kinds of plums, of which some are growing on their own roots, and the rest on the roots of peach trees. The plum trees on peach roots color from ten days to two weeks earlier than those on plum roots, and they also drop that much earlier. In the fall of 1892 the plum on peach roots colored more highly than that on its own roots, but the past fall there was little or no difference in the intensity of color.

The leaves on the outer branches of our dense growing trees color and drop off while the inner ones are still on and green. This is very noticeable in the hard maple and may be seen in the elm and ash to some extent. I have frequently seen hard maples with the naked twigs sticking out two or three feet all round, while the middle of the tree was still green and had dropped scarcely a leaf.

There is a difference in the time of coloring of leaves on trees of the same species, even when they are in perfect health and are under the same conditions. The difference in the time of leafing is frequently spoken of, but I do not remember anyone having noticed that there is a corresponding difference in the season of dropping the leaves in many species. Leaves that color first also drop first; but there does not seem to be any definite relation between the time of leafing and the time of shedding leaves.

There is not only a difference in the season of coloring, but also a difference in the color of individual trees. This difference is very marked in some species, and in others is scarcely noticeable. The ash, elm and walnut each has a characteristic yellow that is comparatively uniform, but other species vary considerably. The hard maple, plum, blackberry, etc., may be seen varying from a pale yellow or orange to a very bright scarlet. The same individuals take the same color, though to the same degree, year after year, just as a variety of apples does, and there is no doubt but that a characteristic season and color could be propagated in the leaf of the maple just as in the fruit of the apple or pear.

It is a practice, not uncommon among some grape growers, to girdle the vines to make the fruit ripen earlier. This makes a difference of something like a week in the season, and the fruit is larger though not so good in quality. Our own plum trees on plum roots have so far borne no fruit, but plum growers inform me that the Wild Goose and some other varieties when grown on peach roots ripen and are gone before those on plum roots have begun to ripen.

The play song beginning, "The higher up in the cherry tree the riper grows the cherry," expresses a familiar fact that is seen not only in the cherry but more or less in all our tree fruits. Those most fully exposed to the light not only ripen more thoroughly, but ripen earlier and color more highly than those in the shade. Apples that grow in the middle of a dense growing tree never do as well nor attain as high a quality as those exposed to the full light, on the outer branches. Whether it is the light or the place where the apple happens to grow that makes the difference, I do not know; grapes color as well and the quality is as good when grown in bags as when fully exposed. Apples grown on hilly or comparatively thin land, where there is little vegetable matter, are more highly colored and are of better quality than those grown on deep rich soils.

There is a correlation in some species between the color of the ripe fruit and the autumn leaves. Generally the blackberry is a very brilliant autumn plant, but the varieties bearing fruits that are white when ripe have pale yellow autumn leaves. Both the black and red raspberry groups have reddish colored autumn leaves, but there are in both these a few varieties with yellow fruits, and the autumn leaves on these are yellow. The yellow fruited plums also have yellow leaves in the fall, and the varieties that have most red in the fruit also have most red in their autumn leaves. Corresponding differences are also seen in the bark of the young wood. I suspect this is an albinism of plants which is related to the albinism of animals.

The fruit is a modified branch and its leaves, and so we have the fruits and leaves corresponding in these ways:

Fruits vary in their time of ripening; leaves in their time of coloring or ripening and falling.

Fruits in the top of some kinds of trees ripen earlier and more perfectly than those in the body of the tree; leaves on the outer branches of some trees color or ripen and fall earlier than those in the middle.

There is an individuality in and a correlation between the color of the ripe fruit and the autumn leaves.

The girdled grape-vine ripens its fruit earlier than those not; leaves on girdled or otherwise injured twigs, branches, or trees color or ripen earlier than those on sound branches or trees, though they do not always fall earlier. The colored leaves may hang on until the frost takes all off together.

Both leaves and fruits color more highly on hilly than on level land; but color in fruit is an indication of maturity.

We have, then, a relation and a regular gradation: The broken leaf, the girdled branch which colors its leaves or ripens its fruit earlier, the brighter color of the injured tree or vine, the earlier ripening and coloring of the fruit and leaves of the plum tree on peach roots, the earlier and higher coloring of both fruits and leaves on broken land, and the earlier and more complete ripening of both leaves and fruits on the outer branches. Probably the leaves on trees in deep rich soils never would color as highly as on those on the rougher land, but the probability is that if we had a longer season for ripening we would have more color.

Is there in the high leaf coloring an indication not of maturity alone, but of an unnatural maturity that would not be found in trees growing in soil and with conditions best suited to their wants?

THE SPOKEN LANGUAGE OF BOTANY.

BY C. MICHENER, SAN FRANCISCO, CALIF.

SOME months ago there appeared in print, over the initials "E.L.G.," the following:

"The eminent dendrologist, Prof. C. S. Sargent, whose good services in the cause of correct nomenclature have been everywhere recognized, has newly discovered that Halesia, long in use for certain American shrubs, is but a homonym, and in a recent issue of *Garden and Forest* (vol. vi., p. 434) has offered Mohria as a substitute. In this choice he does not discover that he has made another homonym; but this is certainly the case, for, as a spoken name—and the language of plant nomenclature is unquestionably a spoken language—*Mohria* is identical with *Morea*, a name already twice employed, first by Medicus and afterwards by Salisbury . . . "—*Erythea*, I. 236.

And thereupon the author, *for the reason he has stated*, publishes a new generic name for Mohria.

Later (*Erythea* I. 246), the same author says: "Maintaining the ground we took that Mohria is at best but a homonym of Morea, we offer the following instead of Swartz's Mohria:" — thereupon again publishing a new generic name.

These quotations are given to show the importance of the question which I wish to present in this paper—a question to which there seem to be two answers, one, that indicated by the course of Professor Greene in the paragraphs quoted, and the other, which I propose now to outline.

This question has been suggested before, but the instances cited are the first which I have noticed that tend toward an active carrying out of an answer to it; and it is in the hope that that solution of the problem may not be adopted—believing as I do that it would lead to ultimate confusion and the injection of the personal equation to a greater degree than ever into the science of nomenclature —that I write this paper.

Professor Greene has stated that the language of plant nomenclatute is unquestionably a spoken language. This, I think, is hardly, as yet, true; for my experience has been that the German spoken plant-name language differs sadly from the English, the English from the French, the French from the American and each from the other.

But it has been decided that uniformity is a useful thing in plant nomenclature. It is perhaps less patent, but extremely probable, that such a spoken language would be a useful thing. These true, it follows that that language should be uniform.

We have agreed that we will not represent any two different plants by identical names. To the date of Professor Greene's notes, above cited, that prohibited identity was identity of letters—of spelling. But we have now to consider a second factor of language—usually the more important, here, I think, the less—sound. And his work indicates that when we find two plants represented by words identical in sound, even though the letters that go to make up the names in each case are not identical, we shall substitute for the name latest in point of time another not identical in sound with the former. That seems to be his solution of the nomenclatural problem upon the addition of the sound-factor.

But, as I have said, I should regret to see it adopted. I think it will be admitted that a means which will involve less change in existing postulates to accomplish a desired result is better than a means which will involve greater change. The desired result in this case is to do away with homonyms in a spoken language of botany. The addition of the sound factor is responsible for such homonyms; (for, for the purposes of this discussion, we will assume that under the rules already in force we have done away with all those whose identity is an identity of spelling—of letters) and it seems to me but reasonable that, if we are to introduce this factor, it should be so introduced as not to disturb in the slightest degree the written language already in existence. The means of doing this is plain. All that is necessary is to assign to each of the twenty-six letters which the written language of botany has employed, a separate, distinct and invariable sound, and we have, as a result, a spoken language in which no sound homonyms exist—in this way obviating the necessity of disturbing in the slightest degree the written language already in existence.

To take, for example, the instance above noted. Professor Greene has noticed the two names *Mohria* and *Morea*. So long as the language of botanic nomenclature remains merely written, these are not homonyms. But if we make it a spoken language, then—in Professor Greene's opinion—they become so. This assumption leads us inevitably to the conclusion that in Professor Greene's spoken language of botany the letter *e* represents a sound identical with that represented by the letter *i*, and that the letter *h* has no sound value—and he is thus compelled to disturb the established written language to adapt it to the suggested spoken language.

In the system which I have proposed, the letter *e* would represent a sound distinct from that represented by the letter *i*, and the letter *h* would have its own definite sound, a sound present in *Mohria* but absent from *Morea;* and therefore I should not be compelled to disturb the established written language to adapt it to the suggested spoken language.

This is the most important consequence of the spoken language which I here propose, and it seems to me that its importance is vital.

A second result of the application of the principle here suggested is that it would produce a uniformity in this spoken language throughout the world; and until this were done, we, as Americans, would have no right to object to some Finnish or Russian botanist making changes in the written language (which is already common to all of us) because, in his particular tongue there might be too close a similarity in the sound of two plant names.

I have prepared a system of sound-values to be given to each letter in accordance with the above plan, observing the following principles:

(1.) Each letter has invariably the same sound in every combination.

(2.) Each letter has a sound distinct from the sound of every other letter.

I do not submit this schedule at the present time, for the reason that I have prepared it, having in view merely the sounds of the Latin-European tongues, and English and German; and I fear that I may have introduced sounds difficult of pronunciation for those who use other languages than these. The practical selection of the most convenient sound in each case is a matter of considerable difficulty, since many names have been published involving peculiar combinations of letters, many of which we are accustomed to regard as being without sound value.

With regard to accent, the simplest rule I have been able to formulate is this: "All words are accented on the last vowel which is followed by a consonant." This rule seems to work very well in this—it is absolute in its application—uniform—, and yet results in variety in the accent of words, some being accented on the antepenult, some on the penult and some on the ultima. I confess it was with somewhat of a shudder that I broke away from classic traditions in the matter of quantity accent; but the advantages of this over the classic rule are so evident, that I feel that I could in time become reconciled to it.

For one must remember that the Latin of botanic names is not classic Latin in many cases, and that the application of quantitative rules in the determination of accent is in such cases merely arbitrary.

I have hesitated for some months about publishing this paper, but it has seemed to me at last that, if the question of a spoken language of botany is to be raised, it is important to define such a language at its outset; and as I have felt that the definition here set forth is one most likely to obviate that potential store of silent synonyms, which must otherwise come upon us, I have suggested it as a basis for improvement.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

The Ling on the Pacific Slope.

THE ling (*Lota lota maculosa*) is found over a wide territory of North America east of the Rocky Mountains. I have taken it in the Great Lakes, at Winnipeg, in the Red River of the North, and it was reported to me at the head waters of the Saskatchewan, where it is said to ascend the smaller streams during the spawning season in such numbers that many could easily be killed by a single discharge of a shot gun. I have also taken it in the Missouri at Craig, Montana. In short, it is found in all three of the large water basins of the Atlantic slope—the Saskatchewan, St. Lawrence and Mississippi.

At Golden, B.C., on the Columbia, I was told by a fisherman that in Autumn he had caught ling five feet long on night lines, but I secured no further evidence of their occurrence in the Columbia system during my explorations in that region in August, 1892. When I reached Sicamous on Shuswap Lake in the Fraser system I was at once asked if I had secured any ling, of which they had some for dinner. As described to me, this ling appears to be a species of *Lota*. I did not succeed in getting any specimens at this place, as the Indians, who alone knew where to take them, had left on the morning of my arrival.

Since then I have received a large specimen from Golden, B.C., on the Columbia, which was secured for me by Mr. Green, manager of the Queen's Hotel at that place. A comparison of this specimen with one from Lake Michigan does not show any specific differences. The known distribution of *Lota lota maculosa* is therefore extended to the Pacific slope.

CARL H. EIGENMANN.

Bloomington, Ind.

The Native Calendar of Central America and Mexico.

I THINK it necessary to notice one or two errors in Dr. Brinton's article in *Science*, Feb. 16, for it seems he has made precisely the mistake he attributes to me. But first it is proper to say that, as his reference to Dr. Seler's explanation of *chic-chan* relates to a different work from that referred to by me, my criticism on this particular point was inapplicable.

If Dr. Brinton will examine my article in the *American Anthropologist*, he will see that my reference to the month names is limited expressly to those of the tribes of the Maya stock, hence his reference to the Nahautl names is out of place. It would be well for reviewers to read carefully and make themselves acquainted with that which they are reviewing.

And again Dr. Brinton has wholly missed the point in my reference to the Java week. I certainly thought it was so clear that a person with but half an eye would see that the singular fact alluded to was that the Javanese, in assigning the days of their week and certain colors to the points of space, like the Zunis assigned a *mixed color* to the focus. A similar assignment of mixed color to the centre is seen on plate 12, Borgian Codex.

Further comparison of the Polynesian calendar with that of Mexico and Central America will be seen in a Bulletin now in press and soon to be issued.

CYRUS THOMAS.

BOOK REVIEWS.

Nagualism. A Study in Native American Folk-Lore and History. By DANIEL G. BRINTON, A.M., M.D., LL.D., D. Sc., Professor of American Archæology and Linguistics in the University of Pennsylvania. Philadelphia, David McKay. 1894, 65 p., 8°, $1.00.

"NAGUALISM" is a word which the reader will search for in vain, even in the Century Dictionary, although Dr. Brinton points out that it has been in occasional use in English and American books for seventy years past. It means the doctrines taught by the sect or secret society of the "Nagualists," who for more than three hundred years have perpetuated in Mexico and Central America many of the superstitions and rites of their ancestral heathenism, strongly infused with a debased Christianity; which did not prevent a cordial hatred of that religion and of the race which introduced it from being a cardinal maxim in their creed.

The Nagualists were also adepts in occult art, as skilful jugglers as those of India, telepaths, mesmerists and were-wolves. They had a secret slang or *argot* of their own, full of dark references and symbolic expressions, examples of which are given. The scenes of their mystic rites were glens and caves, where they held licentious orgies or ascetic penances. In the arts of divination and medicine they were acknowledged masters, and their horoscopes, founded on the ancient native calendar, were accepted with blind faith.

Historically, they played an important part in the history of the country, as they were the instigators of nearly all the revolts of the natives against the Spanish power, a fact overlooked by previous writers, but clearly enough shown in this volume. One of the most remarkable facts brought out is the prominence accorded to women in this secret order. They seem to have been the leading spirits, entrusted with its fullest powers, and often to have controlled its most momentous actions.

It will be seen that the subject of this monograph is an entirely new one, and of unusual interest.

Text-Book of Comparative Geology. By E. KAYSER, Ph.D., Professor of Geology in the University of Marbury. Translated and Edited by PHILIP LAKE, M.A., F.G.S. London, Swan, Sonnenschein and Co.; New York, Macmillan and Co. 1893, 596 illustrations, 426 p., $4.50

AMONG the numerous valuable additions to geological literature in the year 1893, probably none will be better appreciated by English speaking readers than this new text-book of comparative geology. The subject is one much misunderstood and undervalued accordingly, not only by the English geologist, for whom the above work is primarily intended, but also here in America. It is frequently regarded as vain to attempt to correlate palæontological zones, and to compare closely one region with another, yet it is safe to say that without some such broad conception of the science little real progress can be made. It is truly remarked by the translator in his preface that it is only to the use of the comparative

method that we owe the striking generalizations of Neumayr and the philosophical views of Suess. Probably this distrust with which comparative geology is regarded by some scientists is due to the confusion of comparative geology proper with the efforts made by some "pigeon hole" geologists to bring together the formations in different widely separated quarters of the globe, and to illustrate in convenient tabular form their relations and equivalence. Opinions differ so widely regarding these correlations that probably no such table of equivalence could ever be constructed which would satisfy more than its author. We do not mean that such attempts are wrong in principle, but that they fail of scientific treatment and are necessarily blind to all that would throw a doubt, or disturb their harmony or proportion.

Comparative geology, as a science, however, is far from drawing such definite lines; it rather seeks to indicate the possibility of such equivalence and discusses not only the facts relative to the important questions of fauna and flora, but also marshals into place the physical features of structure and of inorganic grouping as well as of geographical relations. Not that Dr. Kayser has omitted tables of stratigraphical divisions, but these have been composed with greatest care and with the best understanding of the difficulties as well as of the facts which make the tabulated statement at the same time both desirable and to a certain extent possible.

Dr. Kayser's "Lehrbuch"[1] appeared in 1891, and was immediately accepted by German geologists as a most important addition to their literature, and since that time has been universally regarded as the standard work upon the subject. The original work, which was intended for the use of German students, was naturally largely devoted to the study of the German formations, but even with this preference clearly marked there was yet more detail of extra German countries than had been usual in previous writings. Nor is this particular attention to Germany necessarily a fault, as in that country most detailed study has been given, and no special locality could have been better chosen as an illustration of continental Europe as a whole. Moreover, the author's intimate personal knowledge of the localities described gives additional satisfaction to the reader. Professor Lake, with a full realization of the peculiar insular seclusion of British geologists, has appreciated the value of this work to his fellows and in giving even a pure translation would have contributed a book of greatest value. He has done more than this, however, in his many additions descriptive of countries outside of Germany, particularly, of course, in treating of England, where, as in the former country, the geological series have been most carefully worked out. These additions are particularly numerous in the early portions of the work, becoming somewhat more limited in the discussion of the later formations, owing to the exigency of space. Doubtless in a second edition the writer will be permitted by his publisher to develop the work in proper proportion. As regards the American formations, the comparisons, though brief, are carefully drawn from our best authors, and the data are quite sufficient for their purpose. Further, as has been suggested in a recent review of this same work by Prof. R. D. Salisbury, data concerning American geology are more easily accessible to American students, thanks to the correlation essays of the United States Geological Survey, than data concerning European geology, which this volume supplies. Particularly is this true of the literature relating to the geology of the continent, which appears without exception in languages foreign to us and consequently difficult for reference.

[1] "Lehrbuch der Geologischen Formationskunde," Dr. Emanuel Kayser. Stuttgart, Ferdinand Enke, 1891.

Dr. Kayser is fortunate in his translator, Professor Lake, who has already attained a high position by original work in the same field, both at home and abroad, in connection with the Geological Survey of India. Mr. Lake has aimed to edit as well as to translate and together with the hearty coöperation of the author has brought the book fully up to date, at the same time introducing an extension of field, as has been indicated. In points of variance between continental and English teachings a review of the divergent tenets has in most cases been given, and where exceptionally unqualified statements have been made, though we may not be in full accord with the author, we must at least acknowledge that these statements represent the beliefs most widely accepted by the geologists of to-day.

We can give but a brief review of the contents of this volume, merely indicating the general treatment of the subject. An introduction defines the subject and its sub-divisions, the classification of the sedimentary strata, with a few words on the origin and early condition of the earth. Part I. treats of the Archæan or primitive rocks, their general character, mode of occurrence, origin, etc. The importance of this great group is fully recognized, though the space devoted to its discussion is rather limited. The Palæozoic or "Primary Group" is discussed in Part II., the order of discussion being that adopted throughout the work for each group and for each system, namely, after general remarks an historical summary, followed by a study in geographical grouping, of development and palæontology. It is in the treatment of the Palæozoic that the translator makes his greatest departure from the author, rejecting, here, the latter's division into the Cambrian and Silurian systems in favor of the English divisions of Cambrian, Ordovician, and Silurian. The Silurian of Lake, as is the custom of English writers, extends to the top of the Salina, or Waterlime, series, the Lower Helderberg and Oriskany being included in the second system of the Palæozoic, the Devonian.

The illustrations, which have been drawn in all cases from the original work, include seventy-three plates and seventy figures in the text, or, in all, 596 illustrations, and these form an invaluable part of the volume. The majority of these, which are all well executed, are devoted to palæontological subjects, but we have also many illustrations of structural and physical conditions. The typographical work is excellent, the choice of type, following the German edition in plan, is such as to bring out at a glance the subject matter of each paragraph and thus to add greatly to the value of the volume as one of reference. This is a matter often either neglected by the author or unsatisfactorily accomplished by the publisher, and it is rare that we find so carefully executed a plan as is here observed.

Edward Livingston Youmans; a Sketch of His Life. By John Fiske. New York, D. Appleton and Co. $2.

This is a book which all who are interested in the diffusion of scientific knowledge among the people, and especially all who knew Youmans personally, will like to read. It is written in the clear, smooth style of which Mr. Fiske is master, and as he was one of Youmans's most intimate friends, the biography has the authority as well as the interest which personal acquaintance gives. The most interesting chapters to us have been the earlier ones, which relate the story of Youmans's early life, his work on the farm and his struggles to obtain an education. Then follows the pathetic account of his blindness, which, though most of the time only partial,

almost disabled him from intellectual work during a large part of his early manhood, and sometimes brought him to the verge of dispair. Nevertheless, it was while suffering from this great affliction that he succeeded with some help from his sister in preparing his text-book on chemistry, which at once made him known to the public, and gave him the means of support. This treatise was, in Mr. Fiske's opinion, Youmans's germinal book, all his subsequent work being foreshadowed in it. Next came his career as a scientific lecturer, in which he was successful not only in a pecuniary way but also in increasing the popular interest in the physical sciences.

The rest of the book before us is devoted to those wider enterprises in which Youmans engaged, beginning with his first acquaintance with Herbert Spencer and his agency in securing the publication of Spencer's works in this country. This part of the book is enriched by a large amount of correspondence between the two men, which not only shows their business relations to each other and to their publishers, but reveals many interesting traits of character. There are also letters from Huxley, Tyndall and other scientists, besides many that passed between Youmans and the members of his family. A full account is also given of the establishment of the *Popular Science Monthly*, which, it seems, was successful from the start, and also of the International Scientific Series, which was not started without some difficulty, but which, as our readers well know, proved both scientifically and commercially successful in the end.

The book closes with a series of extracts from Youmans's writings, giving his views on education, on the relations between religion and science and on other subjects; so that readers have before them as full an account of the life and work of the popular scientist as they are likely to need, and one that is worthy of its subject and of its author.

Elements of Chemistry, Descriptive and Qualitative, Briefer Course. By JAMES H. SHEPARD. Boston, D. C. Heath and Co. 1863, 240 p.

THE success of Shepard's Inorganic Chemistry, as exemplified by its use in over three hundred and fifty colleges and schools, and by the high words of praise which have already been accorded it, is sufficient warrant of Professor Shepard's understanding of the needs of a school text-book in chemistry. The present volume is prepared as a briefer course and is intended to meet the needs of secondary school instruction, particularly where chemistry as a study has been forced into a limited period. In addition to the elements of inorganic chemistry, the author has added to the completeness of the work by a chapter on organic chemistry, treating successfully in an elementary manner this rather difficult department. The book is clear, concise, and well adapted for young pupils. It is interesting to note that Professor Shepard's Inorganic Chemistry has been recommended by the Committee of Secondary School Studies, appointed at the meeting of the National Educational Association.

NOTES AND NEWS.

A CROWDED meeting of the members of the Victoria Institute and their friends took place recently at London, England, in the Theatre of the Society of

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Arts, to hear Dr. Prestwich, F.R.S., read a paper "On a Possible Cause for the Origin of the Tradition of the Flood," in which he "treated the subject from a purely scientific standpoint." The chair was taken by Sir George Stokes, Bart., F.R.S. On account of recent indisposition Dr. Prestwich was unable to travel up to London to be present, and had deputed Prof. Rupert Jones, F.R.S., to read the paper for him. The paper described at considerable length the various phenomena which came under the author's observation during long years of geological research throughout Europe and the coasts of the Mediterranean. He concluded by giving the reasons why he considered that these were "only explicable upon the hypothesis of a widespread and short submergence of continental dimensions, followed by early re-elevation, and this hypothesis satisfied all the important conditions of the problem." The age of man was held to be divided into Palæolithic and Neolithic, and he considered rightly so. He concluded by saying that thus there seemed cause for the origin of that widespread tradition of a flood. The paper was followed by reference to a communication from Sir W. Dawson, F.R.S., who welcomed the paper as confirming his conclusion, come to on geological and palæontological grounds, as to a physical break in the anthropic age. The evidence of this was afforded by the cave remains and from a vast quantity of other sources. The discussion which ensued was joined in by a considerable number, including Dr. Woodward, F.R.S., President of the Geological Society—who, in referring to Dr. Prestwich, regretted the forced absence of the Nestor of Geology, and expressed the hope that ere long he might be able to be present at another meeting of the Institute—Sir Henry Howorth, F.R.S., and Profs. T. Rupert Jones, F.R.S., T. McK. Hughes, F.R.S., and E. Hull, F.R.S.

—The latest volume in Appleton's International Education Series is by Susan E. Blow, on "Symbolic Education," being a commentary on Froebel's "Mother's Songs and Games." Miss Blow begins with an attack on Rousseau, which is in the main well deserved and effective; but we cannot agree with the opinion expressed at the end of the chapter that "between the views of Rousseau and those of Froebel there are in my judgment no affinities whatsoever." For the fundamental principle of Froebel's method of teaching is that the child's mind follows a natural course of development to which education, if it is to be successful, must conform; and this principle he obviously borrowed of Rousseau. Nor can we agree with Miss Blow's extreme views about the fondness of children for symbolism and the effectiveness of symbolic teaching in inculcating moral truth and forming virtuous habits. The old symbolic religions were certainly not very successful in improving morality, and we believe that such improvement in children can be much more effectively promoted by direct teaching and the influence of example than by any form of symbolism. In treating of Froebel's practical devices as employed in the kindergarten, Miss Blow has many things to say, some explanatory and some critical, which kindergarten teachers will find interesting and doubtless profitable; but we have no space to enlarge upon them here. We should add that this book contains but a portion of what Miss Blow desires to say on educational subjects, and that she intends to issue another work at some future time to complete the exposition of her views.

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